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CRYSTALLIZATION-DIFFERENTIATION IN IGNEOUS MAGMAS

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INTRODUCTION

A formal definition of the term magma is seldom given in petrologic literature, but it is plain from most writings that the word is intended to signify completely molten rock. Etymologically magma is perhaps more definitely applicable to a liquid with much suspended solid matter, that is, to a sort of mush. In this sense the term might designate igneous material in which crystallization was well advanced, and, while such usage should perhaps not now be urged, it is none the less true that magma in this latter sense is of fundamental significance in the genesis of igneous rock types. In his paper discussing the crystallization of magmas[1] in which that process was advocated as the fundamental factor in differentiation, the writer pointed out the special importance of two stages, the early stage of crystallization, when crystal settling may occur, and the late stage of crystallization, when squeezing out of residual liquid may occur. Throughout most of the discussion of crystallization-differentiation, however, particular attention was given only to crystal settling. Since the publication of that paper a number of writers have pointed out various features of igneous rocks for which crystallization-differentiation has seemed to them to offer no adequate explanation. These objections have arisen largely from a failure to appreciate the importance to differentiation of the period when igneous material is largely crystalline, that is, the period when it is magma in that sense of the term to which attention is called above.

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[1] "The Later Stages of the Evolution of the Igneous Rocks," *Jour. Geol.*, XXIII (1915), Supplement.

In the present paper the writer wishes to offer suggestions regarding some of the more general objections that have been raised against the theory of crystallization-differentiation and especially to apply it to certain phenomena for whose explanation it has been considered inadequate.

RELATIVE IMPORTANCE OF SYNTEXIS

In a recent paper on the "Genesis of the Alkaline Rocks" Daly discusses many features of the genesis of igneous rocks in general[1] and takes the opportunity of criticizing certain aspects of the theory of crystallization-differentiation. In his introduction and at other points in the discussion Daly makes statements that convey the impression that that theory, as a whole, must stand or fall with its ability to explain the alkaline rocks. A brief examination of the basis on which the theory rests will show, however, that such statements are hardly justified. From the results of a series of experiments that dealt with the crystallization of silicate melts the writer was led to the opinion that igneous rock types as we know them in the field could be developed as a result of the fractional crystallization of basic magma under appropriate conditions. The actual experimental work was carried on with mixtures of simpler constitution than the usual igneous rock, but frequently a fairly close approach to the composition of an igneous rock in its more essential aspects was accomplished. In such instances the conclusions drawn from the experimental work may be considered to be of correspondingly greater reliability. If we examine the experimental data we find that such close approach was made only in the case of the subalkaline rocks, and in the development of the theory of petrogenesis it was only after these data had been applied to the problems of the subalkaline rocks that a possible solution of the alkaline rocks became apparent. In short, then, the conclusions arrived at in the case of the subalkaline rocks represent but a short extrapolation from the experimental data, whereas in the case of the alkaline rocks the extrapolation is a long one and correspondingly subject to error. While there is no reason at present for receding from the position taken regarding the genesis of alkaline types, it is quite possible that the position might prove wholly untenable without any necessary doubt being cast on the strength of the hypothesis that the subalkaline rocks, at least, are derived by processes of differentiation in which fractional crystallization is the primary control.

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[1] *Jour. Geol.*, XXVI (1918), 97–134.

The writer's hypothesis, then, refers the diversity of igneous rocks to the differentiation of basic magma through a spontaneous power resident within the magma and resulting from its own crystallization. Daly's hypothesis likewise assumes original igneous material of a basic nature, but considers that it has little power of spontaneous differentiation and that it acquires a significant tendency to differentiate only when equilibrium is upset by the assimilation of foreign material. For each great family of igneous rocks a particular type of foreign material has been assumed to be the special agent. To Daly assimilation is all-important, to the writer it is a mere adventitious circumstance. There is nothing, however, in the writer's hypothesis that justifies Daly's statement that he assumes differentiation to affect only purely juvenile magma without foreign contamination.[1] Daly makes this statement in his discussion of the writer's definition of differentiation, in which discussion he directs his attention more toward the form than toward the substance. In another part of his paper (pp. 88 and 89) the writer describes his conception of the *differentiation of a magma that has suffered foreign contamination.* The interpretation that Daly puts upon the definition, while perhaps not an impossible one in the light of its wording, is rather surprising in anyone who has read the whole paper as carefully as Daly has evidently done. The phrase "without foreign contamination" was inserted in the definition to make it expressive of the general agreement among petrologists to exclude from the category of differentiation those variations of igneous rocks that result purely and solely from assimilation of foreign material. That syntectic magma so formed could not suffer differentiation was no part of the intent of the definition.

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[1] *Jour. Geol.*, XXVI (1918), 117.

In that part of the paper in which he discusses the differentiation of a syntectic magma the writer does, however, state his belief that such differentiation must usually be of limited scope, giving his reasons and describing the limiting factors. Contrasted with this is the assertion of Loewinson-Lessing, to which Daly offers adherence, that "a small degree of contamination with foreign material may change equilibrium in the magma, which, therefore, separates into strongly contrasted parts."[1] Such a generalized statement leaves one wondering as to the nature of the equilibrium referred to. Between what is the equilibrium? What is the nature of the strongly contrasted parts? Are they solid, liquid, or gaseous? Do rocks show the evidences of such processes? Have analogous processes been encountered in experimental science, and is the analogy of real significance?

In going on to a discussion of differentiation Daly proceeds to enumerate what may be called the units of differentiation. Some of these are presumably the strongly contrasted parts formed as a result of the disturbance of equilibrium mentioned above, but no particular mention is made of those that are to be so regarded, nor is any discussion offered of the manner in which solution of solid rock causes the separation of the units.

The units enumerated by Daly are the following:

1. Contrasted fluid phases of an initially heterogeneous magma, including parts particularly rich in volatile constituents.[2]
2. Solid crystals (fractional crystallization).
3. Mother-liquor left after partial crystallization.

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[1] This is Daly's form of expression of the Loewinson-Lessing idea.

[2] Units of type 1 need not be considered, for according to Daly's own statements on the page immediately preceding, "rock phases resulting from originally different parts of the magma can be called differentiates only by destroying the useful definition of differentiation already adopted by most petrologists," and in this the writer concurs.

4. Non-consolute liquid fractions (liquid immiscibility).

5. Material of fused country rock, not diffused into the original magma (ultra-metamorphism in part).

6. Original magma locally charged with material dissolved from the country rock but slowly diffusing from the source of supply (syntexis).

Units 5 and 6 are plainly produced by the acquisition of foreign material but cannot be regarded as a separation of the original magma into contrasted parts as a result of this acquisition. The contrast is rather between the original magma and the included foreign material. No doubt such units are real factors in differentiation, but one may reasonably question whether they are of great importance. Differentiates formed from such material would have no necessary consanguinity with other differentiates formed from the magma itself, since their composition would depend principally on the nature of the included material, that is, on a purely accidental circumstance. It is, however, the fact that differentiates do normally show consanguinity that has led to the whole conception of differentiation in petrology. A differentiate formed by the accumulation of these clots of foreign material would necessarily show a very simple chemical relationship with the available foreign material. It is the fact that this simple relation does not exist that led Daly himself to reject purely marginal assimilation, and that should furnish an equally cogent reason for rejecting accumulation of xenolith material as an important factor.

Daly's case is much stronger in this respect when he assumes complete assimilation (usually abyssal) of the foreign material and subsequent differentiation of the syntectic magma, for then all the differentiates would show the requisite consanguinity. We are thus brought back to a consideration of the separation of phases (in the more definite physicochemical sense of the term) as the fundamental factor in differentiation, and the possible units then become those enumerated by Daly as 2, 3, and 4. The processes involved, namely the separation of a crystalline phase or of a non-consolute liquid phase, must be regarded as possible factors in the differentiation not merely of Daly's syntectic magmas but of normal uncontaminated magmas. Daly considers differentiation as particularly prone to occur in syntectic magma because "equilibrium is upset" by the foreign material. The writer, on the other hand, gives his reasons for believing that the chilling effect normally consequent upon the immersion and solution of foreign material in a magma is sufficient to limit very materially the differentiation of magma that has been so affected,[1] and thus arrives at substantial agreement in this respect with Harker when he makes the statement, "Hybrid rocks are barren." In this connection it seems desirable to consider a statement made by Shand, to which Daly refers with approval.[2] The statement is that foyaitic or phonolitic magmas may enter "into chemical combination with the silica of invaded rock masses. *The reactions thereby induced would be exothermic and would tend to raise the temperature of the magma. The access of heat produced in this way would in turn enable the magma to perform a further amount of work in the way of mechanical solution.*"[3] In short, one might say that the addition of solid rock to a magma is, in this case and possibly in others, a mere adding of fuel to the flames. The conception probably had its origin in the long-used terms "acid" and "basic" as applied to igneous rocks, and in an unconscious extension of the analogy implied in these terms. It is true that when an acid is added to a base they unite with avidity, so much so that one must make the addition with care on account of the consequent rise in temperature. No such care is necessary when silica is added to molten nephelite.[4] Even at a temperature of 1550° C., where the product of the reaction (albite) is superheated over 400° C., silica dissolves in nephelite with excessive reluctance. Indeed experimental study suggests that the following general equation could be written with some confidence: molten rock + solid rock = molten rock $-x$ cal. It is not improbable, however, that, in some cases at least, the following equation would hold: molten rock + molten rock = molten rock $+x$ cal. In other words, it may be regarded as not improbable that two molten magmas might in some cases mix with evolution of heat, but that the solution of solid rock in molten magma is ever accompanied by evolution of heat may be regarded with extreme doubt. It is this reasoned conviction that has led the writer to limit the importance of assimilation.

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[1] "The Later Stages of the Evolution of the Igneous Rocks," *Jour. Geol.*, XXIII (1915), Supplement, pp. 85-86.

[2] *Jour. Geol.*, XXVI (1918), 110.

[3] Italics are mine.

[4] N. L. Bowen, "The Composition of Nephelite," *Amer. Jour. Sci.*, XXXIII (1912), 50.

LIQUID IMMISCIBILITY AS A FACTOR IN DIFFERENTIATION

Having in mind then the factors that may be considered to restrict the formation and differentiation of syntectic magmas, we may now return to the question of the separation of phases as the agency of differentiation of magmas, presumably dominantly juvenile and uncontaminated. The possible phases are, as we have seen, the units 2, 3, and 4 of Daly. Units 2 and 3 are, of course, necessarily concomitant, and to them the writer assigns the fundamental rôle in differentiation, but Daly is inclined to belittle their importance in favor of unit 4, that is, non-consolute liquid phases. The writer considers these to be non-existent in silicate magmas, this conclusion being based in part on experimental evidence. Not a single example of liquid immiscibility among silicates has been encountered in experimental work, in metallurgical practice, or in any of the many industries where molten silicates are treated. The single example of immiscibility of which there is good evidence in natural rocks, that between silicates and sulphides, is promptly revealed experimentally and in metallurgical practice. To many, however, the experimental evidence will never be convincing. When the range of experimentation with silicates shall have been extended to pressures of 20,000 atmospheres it will be possible to state that at pressures of 30,000 atmospheres wholly different phenomena might occur. Or again, an experiment made by human hands will probably never be prolonged to such a degree that its duration might be measured in centuries, so that it will always be possible to say that the time factor was not adequately considered. However, it is desirable to give some consideration to that experimental evidence which Daly brings forward, and which seems to him to indicate the probability of immiscibility under high pressures. The suggestion is that high pressure may induce undercooling, and that immiscibility may result from the undercooling. This suggestion arose from experiments made by Bridgman, in which undercooling of liquids became more marked under higher pressures. Daly imagines that magmas may become similarly undercooled under pressure, and that in the undercooled condition unmixing may occur. Assuming the undercooling, it must be admitted that a certain possibility of the formation of non-consolute liquids is thereby introduced, for examples are known in which mixed liquids that are otherwise completely miscible develop immiscibility when undercooled. But such liquids reveal this peculiarity by having a freezing-point curve of a peculiar and characteristic shape.[1] Curves of this shape, or even remotely suggesting it, have not been noted among the liquidus curves for silicates, so that it may be regarded as unlikely that undercooling can develop immiscibility in silicates.

Quite apart, however, from the question of the probable effect of undercooling, we may point to the complete failure of igneous rocks to show evidence of undercooling when occurring in the largest masses available for human inspection, viz., batholiths. Indeed, if we consider the series that begins with a glassy lava, passes through a glassy lava with spherulites, through felsitic and fine granular textures, and ends with a coarse granular panidiomorphic texture, we find definite evidence of a continuous decrease in the degree of undercooling under which crystallization took place. The abyssal rocks are therefore characterized by a decidedly limited degree of undercooling, and the great magma reservoirs (if by this is meant something deeper than batholithic masses that become exposed) are exceedingly unlikely seats of significant undercooling. All of this is in accord with the laboratory experience that if one wishes to undercool a substance he must use a small quantity of it. The particular conditions not covered experimentally and urged by Daly as possible conditions favoring immiscibility do not, therefore, seem promising in that direction.

The writer's second reason for rejecting immiscibility is not adequately stated by Daly. He states it as being the "high melting temperatures of olivine, magnetite, and other components of monomineralic rocks." This is in reality a collateral consideration, the reason being rather that natural rocks fail to show the kind of evidence that they would of necessity show were immiscibility a fact; indeed, it is incredible that anyone who has followed the process to its inevitable consequences could favor immiscibility in magmas.

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[1] Boeke, *Grundlagen der physikalisch-chemischen-Petrographie*, p. 113.

A feature of igneous rocks that has led Grout,[1] Daly, and a number of others to favor immiscibility is the fact that two adjacent rocks that are evidently closely related frequently show a very abrupt transition from the one to the other. Yet a brief consideration of liquid immiscibility should show that it is not as likely to give discontinuous variation as is crystallization. It is true that if two liquids that are only partially miscible are shaken together in a flask two different liquids are formed, and if the flask be set aside they will become two separate layers with a definite bounding surface. If the temperature is kept constant these two distinct and sharply bounded layers will persist. However, if the immiscibility is the result of cooling a homogeneous solution the behavior is not so simple. In this case a certain amount of immiscible globules should form in the liquid when a certain temperature is reached, and, even if time were allowed then for the collection of the globules as a separate layer, more immiscible globules would form *in each layer* as soon as cooling was resumed. And when cooling had proceeded to the point where crystallization ensued a marked increase in the separation of immiscible globules would occur in association with, and as a necessary consequence of, the separation of crystals. We thus see that immiscibility is not a process taking place at an early stage of cooling as a result of which a sudden separation of a liquid into two liquid layers occurs. The separation is rather a formation of small globules that grow slowly by diffusion and can collect as a separate layer only by comparatively slow movement in response to gravity. Neither is immiscibility a process that is completed at a very early stage in the cooling history, and of which all evidence is destroyed. It is a process that may begin very early but must continue until the later stages of crystallization, and the evidence of it would be as obvious and unfailing as the evidence of crystallization itself. The complete collection of *all* the immiscible liquid as a separate and distinct layer is as unlikely as the complete collection of a kind of crystals whose separation continues until a late stage.

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[1] "A Type of Igneous Differentiation," *Jour. Geol.*, XXVI (1918), 656.

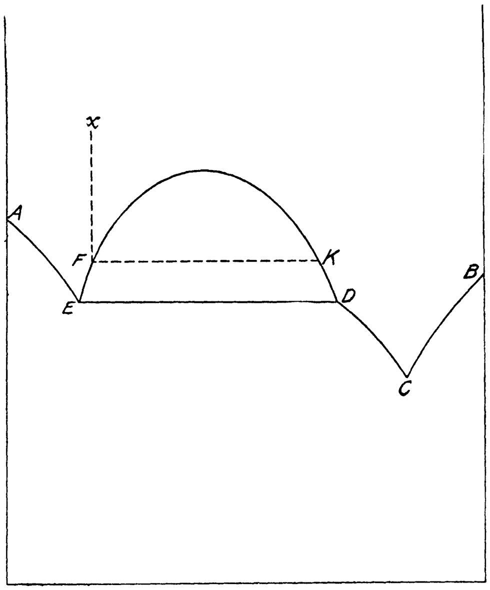

FIG. 1.—Illustrating behavior of a binary mixture with partial miscibility.

We may perhaps make clear these facts regarding immiscibility by discussing the simplest possible binary example. Figure 1 presents the temperature-composition relations. When a liquid of composition x is cooled to the temperature FK, liquid of composition K, that is, a liquid rich in B, begins to separate from it, and as cooling proceeds the composition of the one liquid changes along FE and of the other along KD. The liquid represented by points on FE decreases in amount, and that represented by points on KD increases in amount. The first separation of liquid must be represented by the formation of minute nuclei that grow to larger and larger globules as the cooling proceeds, and as a result of the slow diffusion of material to these globules. There is no reason why this process should be accomplished any more rapidly for separated liquid than for separated crystals. If the separated globules were heavier than the general mass of liquid they would sink, and here enters the possibility of the growth of these globules to much larger dimensions than crystals, because two globules encountering each other may coalesce. The formation of very large globules in this manner would result in their more rapid accumulation as a separate layer. It should be noted, however, that this rapidity of accumulation could never result in the complete accumulation of all the globules as a separate layer. If, for example, cooling were interrupted at some temperature between *FK* and *ED*, and time allowed for the accumulation of all the globules as a separate layer, as soon as cooling was resumed new globules would form in each layer, and their accumulation by the slow process of gravitative adjustment would begin again. It is plain then that whatever complications are assumed the magma must arrive at the temperature *ED* in a blotchy condition, many of the blotches being of rather large dimensions as a result of the coalescence of globules. By large dimensions is meant a diameter several times, perhaps very many times, the diameter of the crystals in the average plutonic rock. At the temperature *ED*, when the liquids in equilibrium have the composition *E* and *D*, crystallization begins, crystals of *A* separating. It is important to note the nature of the first crystals separating, for it will be recalled that the liquid separating was rich in *B*. Those who advocate the separation of olivine, pyroxene, plagioclase, etc., as immiscible liquid tacitly assume a correspondence between the kind of material that would separate early as a liquid and the kind of material that we know from experimental and petrographic experience separates early as crystals. As a matter of fact there is no necessary relation, and the fact that correspondence must be assumed in each individual case is sufficient in itself to throw doubt on a process requiring such an assumption.

Continuing the consideration of the cooling of the mixture, which had been carried to the stage of the beginning of crystallization, at the temperature *ED*, we find that crystals of *A* would separate, and that as a necessary consequence more liquid of composition *D* would be formed and some liquid of composition *E* would be used up. These reactions would continue at constant temperature with the amount of crystals *A* and the amount of liquid *D* increasing at the expense of liquid *E* until finally all of liquid *E* would disappear, when the whole mass would be made up of about 80 per cent of crystals *A* and 20 per cent of liquid *D*. We thus see that up to a time when the mass is largely crystalline two liquids are present, and the crystalline product cannot fail to show the blotchy condition that this predicates. The evidence of immiscibility would not be confined to rapidly chilled flow and dike rocks alone, though it would presumably be especially clear in them. Further cooling would result simply in the separation of more crystals of A with a consequent change in the composition of the liquid from D to C, where eutectic crystallization of both A and B would occur. It will be noted too that the liquid C, which is the *last* material to crystallize, is closely related in composition to the liquids K-D, the *first* material to separate as a liquid. This is important in connection with the well-recognized parallelism between "Differentiationsfolge" and "Kristallizationsfolge." If liquid immiscibility were a prominent factor, or even a subsidiary factor, in the differentiation of igneous rocks no such parallelism would exist.

If we return now to those discontinuous changes of composition in associated igneous rocks that have led to the suggestion of immiscibility, we may make the discussion more concrete by referring to a specific case for which immiscibility has been invoked, viz., the case of the Duluth laccolith. Grout has concluded that the material of this great laccolith or lopolith, as it has been called, was intruded as a homogeneous magma, that crystallization-differentiation controlled by convection currents and gravity ensued, whereby the peridotite and banded gabbro were produced, and that when a certain stage of crystallization had been reached the material of the red rock separated from the gabbro as an immiscible liquid.[1] The first stages of this process, with modifications that are perhaps not fundamental, the writer is able to agree with, but he can see nothing in the described Duluth rocks to warrant the assumption of immiscibility, nor indeed in any other igneous series that shows this association of gabbro and granophyre without intermediate types. The kind of immiscibility assumed by Grout, namely that which ensues only after a certain amount of crystallization has occured, is in no sense different from that which we have discussed. Indeed some of the liquids of the system described show this behavior, viz., those having a composition lying between A and E (Fig. 1). This liquid, which formed only after crystallization was well advanced, would form throughout the gabbro, and some of it would have to rise through several thousand feet. Granting that this was possible at a certain stage and complete accumulation of the liquid took place, what has become then of the further quantity of like liquid that must have separated when the conditions causing its first separation became intensified, that is, when crystallization of the gabbro was carried farther? Unquestionably clots of such material should occur everywhere throughout the gabbro. When it is realized too that experimental studies have shown beyond peradventure that a material rich in alkalic feldspar and free silica is a possible crystallization residuum of a gabbro magma and especially of an olivine gabbro the assumption of immiscibility is seen to be quite gratuitous. This is more especially true for that great number of examples where the acid material occurs in part as granophyric interstices in the gabbro phase, for there it is plainly a crystallization residuum occupying crystallization interstices and not occurring as the clots that one would expect of an immiscible liquid.

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[1] *Jour. Geol.*, XXVI (1918), 658.

CRYSTALLIZATION AS THE CAUSE OF DISCONTINUOUS VARIATION

This association of gabbro and granophyre without intermediate types, of which Grout has collected a number of examples from the literature, is a feature of igneous rocks well recognized by the writer.[1] It is true that in discussing crystallization-differentiation it was shown that a complete series might be formed, extending from gabbro to granite with every intermediate step, but there is no necessity that the series be complete. Only in a very large mass is such a series to be expected, where not only the great size but conditions of comparative quiet concurred to produce great freedom in the settling of crystals. In masses of moderate size the differentiation, if it can be so called, may be a localized affair occurring about each individual crystal, where the separation of the earliest crystalline material from its mother-liquor is accomplished not by its sinking but by the formation of zones about it. In this case the late differentiate occurs merely as interstitial material. If this zoning action is combined with only a limited amount of sinking of

[1] N. L. Bowen, "The Problem of the Anorthosites," *Jour. Geol.*, XXV (1917), 227.

crystals one can obtain a mass showing the last liquid as an upper layer and also as interstitial material in part of the rest of the mass. This is likely to give a pair of rocks showing that marked contrast between a granophyre on the one hand and a diabase or gabbro with only granophyric interstices on the other. Daly has failed to grasp the significance of zoning in producing granophyric interstices and seeks to destroy the whole theory of crystallization-differentiation at one fell blow by pointing out that pyroxene, olivine, and plagioclase have not sunk out of granophyric diabase.[1] It is not necessary that they should, for the prevention of reaction between crystals and the liquid from which they separated may be accomplished, as we have seen, not only by a spatial separation but also by a localized mechanical separation due to zoning.

There is, however, a third method of separation of liquid from crystals that is probably the principal agent of production of those discontinuous variations frequently seen in rock series and particularly that shown in the gabbro-granite (granophyre, etc.) association. It is the squeezing out of residual liquor at a stage when the mass is largely crystalline, and it is many times more promising as a possible cause of discontinuous variations than is any process of limited liquid miscibility, which, as we have seen, is a continuous process requiring the slow action of gravity as an ally and not significantly more competent than crystal settling to produce discontinuity. This squeezing out of liquid is a process that is at first thought difficult to visualize, yet it is one that must be a very real factor in igneous rock genesis. It is the sort of thing one may see at any time when walking on the wet sand of a beach. The foot leaves a slight imprint in the sand that is surrounded by an area where the surface of the sand is particularly wet. The mass as a whole is sensibly incapable of flow, so that the pressure of the foot results merely in a more efficient close-packing of the sand grains coming immediately under its influence, with a consequent squeezing of water into the surrounding sand, where some of it exudes upon the surface. On the other hand, in very wet sand or in mud the whole mass may flow under the foot. The filter-press action exhibited by the sand is but roughly analogous to the phenomena possible in a partly crystallized magma but may serve as a starting-point in the discussion. The relative movement of liquid with respect to crystals by this general method is probably not normally possible with rock materials before the crystals have grown together into a mesh. In order to discuss the action here suggested we shall assume specific cases and consider, as well as may be, the mechanics of the process.

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[1] *Jour. Geol.*, XXVI (1918), 121.

Laccolith with upper late differentiate.—Assume that a sheetlike mass of magma is injected between two members of a sedimentary series, with slight arching of the upper member so that a flat laccolithic mass is produced. The fact that the sheet was injected at that horizon may be regarded as being probably determined by the competency of the member immediately above it. If this terrane is being acted upon by a compressive thrust the tendency will be to shorten the lateral dimensions, and the competent member will yield by forming a more pronounced arch, while the relatively incompetent beds immediately beneath the mass will yield by shortening and thickening. This action, going on slowly, will result in a thickening and shortening of the laccolithic mass, which offers no resistance until crystallization has proceeded to such an extent that the crystals touch each other and have grown together into a strong framework. When sufficiently strong this crystal mesh may finally be able to sustain the thrust and cause a pause in the shortening action. The condition then existing might be represented by Fig. 2*a*. The stress may accumulate until finally the crystal mesh begins to break down, whereupon the beds beneath will yield as before, and the competent beds above will bend as before. Likewise, coincident with the beginning of the breakdown of the crystal mesh the interstitial liquid will feel the pressure of the thrust and will transmit it hydrostatically to all parts of the mass, and the vertical

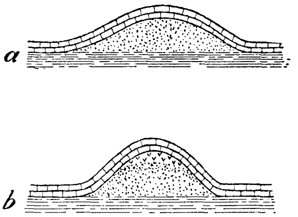

FIG. 2.—(*a*) Laccolith at an advanced stage of crystallization; (*b*) same after being subjected to lateral pressure at this stage. Shows squeezing of residual liquid (late differentiate) into the crown of the arch.

component of this hydrostatic pressure on the liquid will aid in emphasizing the up-arching of the beds of the roof. As the crystal mesh *progressively* breaks down, and the horizontal dimensions are shortened in consequence of closer packing of crystals, there will be a flow of the interstitial liquid into the space developed by the up-arching of the roof. All three actions—shortening of the lateral dimensions by closer packing of crystals, up-arching of the roof, and movement of the liquid—go on *pari passu* and are thoroughly interdependent. When the crystals have become sufficiently closely packed to withstand the thrust the whole action must cease, and of course may cease sooner for other reasons. The resultant mass is therefore, in the simplified case we have assumed, a laccolith showing marked differentiation, with a rock represented by crystalline material separated from the original magma as a lower differentiate, and as an upper differentiate material represented by a late residual liquid (Fig. 2*b*). The differentiation would be markedly discontinuous, the later differentiate would have an intrusive relation to the earlier in places, in other places there would be a rather abrupt transition, and yet the relations would not indicate successive intrusion in the ordinary sense of the term. In so far as this action is incapable of occurring before a certain degree of crystallinity has been attained, there should be a tendency toward a fairly constant contrast between the two differentiates in the matter of composition. On the other hand there is no necessity for constant relative proportions in their amounts. Associated igneous rocks when formed by this method should present, therefore, a fairly constant hiatus as compared with the complete igneous series that is capable of production under other circumstances. Any mass, not necessarily of the simple shape we have assumed but capable of being acted upon in the same general manner, should give a closely related result, so that the general phenomenon might well be of widespread occurrence. The gabbro-granophyre association fulfils every requirement.

An example of the extreme simplicity we have pictured will of course never be found. One of the principal complications results from the fact that the freezing of such a mass will take place from the outer layers inward, except for such modification of this behavior as is introduced by crystal settling. As a result of this more advanced consolidation of outer layers an upper layer may become more or less definitely a part of the upper competent member of the stratified series and may act in conjunction with it during the process of up-arching. The later differentiate would not normally, therefore, be the uppermost. It would, indeed, be located more nearly toward the central part of the mass according to the degree of this freezing inward from the roof.

Sheet with thin central late differentiate.—In certain cases where the freezing has been nearly symmetrical at the top and bottom of a sheetlike mass a horizontal thrust, occurring at a time when there was still a thin layer mostly liquid centrally placed, might cause a general shortening of the lateral dimensions of the sheet, with consequent thickening vertically. These would take place concomitantly and would be accomplished by closer packing of the crystals in the outer layers, where crystallization was well advanced, and a thickening of the originally thin central liquid layer by inflow of the interstitial liquid from the outer layers. There would thus be formed a mass having a thin central later differentiate with an intrusive to transitional relation to its surroundings, and with a distinct tendency toward a streaky development, for the liquid squeezed in from the layers where crystallization was well advanced would differ from the liquid already present in the narrow central band when this action began.

Thick central late differentiate.—There is also a very simple method whereby a sheetlike mass, whether dyke or sill, can develop locally a very thick, centrally placed layer of this late differentiate. It is the warping of the containing walls of the sheet, with consequent thickening of the sheet in a certain place and thinning of it elsewhere. If this takes place at any time except at a late stage of crystallization it will normally have no significant result in the way of differentiation. If it takes place at a time when the crystal mesh has considerable strength it may have important consequences. We shall consider a segment of the sheet including a part that has been thickened. Figure 3*a* represents part of the original sheet and Fig. 3*b* the same after warping of the walls. In the section where the sheet is thinned the crystal mesh will be broken down and the crystals more closely packed. There is an intermediate neutral zone in which the sheet retains its original width and the crystal mesh remains undisturbed. This portion therefore acts as a permeable partition through which the interstitial liquid of the thinned portion passes.[1] As the walls begin to move apart in the thickened portion the crystal mesh will part along a median plane, for there it is weakest, and the interstitial liquid from the thinned portion will flow in to occupy the space afforded. At no time will there be any space in the sense of empty space. The movement of liquid into the space will be absolutely concurrent with, and perhaps a contributing cause of, the development of the space. The action will continue until a proper balance of the forces involved is reached. In certain sections the thickness of the sheet may be increased to several times its original value, and the late differentiate may be a very thick central band, indeed several times as thick as the narrow bands of the early differentiate on either wall. The thinned portions will normally be thinned only a moderate amount, so that relatively large areas must be thinned. The late differentiate would again have an intrusive relation to the earlier material on the walls, with, however, "welded" contacts and locally a transitional relation of a more or less abrupt nature. This action could take place only at a late stage of crystallization, when the crystal mesh had very considerable strength, if only for the reason that the neutral zone must have sufficient strength to withstand a considerable difference of hydrostatic pressure on opposite sides of it, for there would be very great frictional resistance to the flow of liquid.

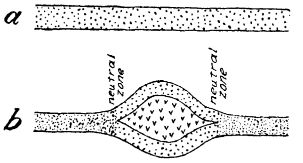

FIG. 3.—Sections of a tabular mass, dyke, or sill: (*a*) at an advanced stage of crystallization; (*b*) after warping of walls at this stage and squeezing of residual liquid (late differentiate) into expanded section.

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[1] For the purpose of presentation of the conception in particularly simple form this definite neutral zone is assumed. Its existence is, however, not essential to the action described. It is merely necessary that the breakdown of the crystal mesh should go on progressively as stated on p. 408. This will bring it about that the portion not yet broken down acts at any instant as a permeable partition preventing the passage of crystals and permitting the passage of liquid from the portion breaking down at that instant.

It is suggested that the various kinds of filter-press action that have been discussed hitherto probably cannot take place before the mass is 80 per cent crystalline. In all cases we should have a marked hiatus in the crystallization series that could have formed under conditions favoring continuous differentiation, that is, conditions favoring crystal settling. Even with the slow cooling and the quiet cooling requisite for crystal settling a certain amount of the filter-press action we have discussed may occur when crystal accumulation has proceeded to the appropriate degree.[1] No general discussion of the combined effect will be attempted. Instead an explanation will be offered for the phenomena exhibited by the special example, the Duluth lopolith.

DIFFERENTIATION IN THE DULUTH LOPOLITH

On this igneous mass Grout has recently published an interesting series of papers. It is a large, sheetlike body mainly of gabbroid composition but showing differentiation, with masses of peridotite at the bottom, a great thickness of olivine gabbro with striking banded effects and anorthositic facies, and at or near the top a so-called "red rock" or granophyre. Grout thinks that crystallization was a prominent factor in the production of differentiation, and that convection was the dominant agent in bringing about the observed distribution of crystals. There was also a wholly different type of differentiation, according to Grout, which involved the separation of the red-rock magma from the partly crystallized gabbro magma as an immiscible liquid. To this latter phase of his conclusions certain objections have already been offered that later will be referred to again. Any type of differentiation in which crystallization is the fundamental factor is, in the writer's opinion, much more likely to occur, and any promising process that may be put forward as conducive to the localized accumulation of crystals is unlikely to meet with opposition from the writer. Convection does not, however, seem to offer any promise. Grout introduces a type of convection called two-phase convection, in which a mass of liquid containing suspended crystals is considered to act as a unit and to sink in virtue of its greater aggregate density. It was evidently suggested by Daly's two-phase convection, in which the two phases involved were liquid and gas that gave a low aggregate density. It is known that masses of foam possess some of the attributes of a rigid body, and it might therefore be expected that localized highly vesicular masses of lava might act as a gravitative unit, but nothing is known to warrant the extension of the idea to a liquid containing suspended crystals. Moreover there appears to be no necessity for appealing to this unlikely process, since ordinary thermal convection would be equally effective and can hardly fail of occurrence in a cooling mass of magma.[1] However this may be, we may now proceed to consider the supposed effects of convection. Grout makes some calculations on the rate of convection which indicate that crystals formed at the cooling portion near the top could be carried to the bottom in half an hour. No reason is offered for this precipitate haste. Undoubtedly many years were available; indeed Daly considers the time available to be embarrassingly long and offers it as an objection to crystal settling that the results are often very meager when the duration of crystallization is considered. Moreover, it should be realized that a current that could carry crystals down in half an hour could carry them up again in half an hour, particularly a current in which a mass of liquid with suspended crystals is supposed to act as a unit. The lagging behind and lodgment of the crystals at the bottom as the currents sweep over the floor, which Grout considers would occur, would undoubtedly be a factor in the case of ordinary thermal convection at any rate (its possibility in the two-phase convection pictured by Grout may be doubted). It should be realized, however, that this occurs in spite of convection and in virtue of the superior density of the crystals rather than in virtue of convection, and the really important question is whether crystals would accumulate at the bottom more effectively by this dragging-behind process than they would by simple settling. In other words, does convection act as an aid or as a deterrent to crystal settling? The answer suggested by experience is that it would act as a deterrent. Sedimentation in nature is always delayed by convection, and in the laboratory when one wishes to make separations by means of heavy solutions one always obtains a more ready separation by jacketing the heavy solution in order to prevent even the very moderate convection that results from the slight draughts in the laboratory.

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[1] A combination of the two actions is probably the most promising explanation of many monomineralic masses.

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[1] It should be noted that the writer has never doubted the reality of convection in magmas. He merely doubts its efficacy in producing differentiation.

Perhaps, however, the more important reason for advancing convection was not its greater efficiency in bringing about accumulation of crystals as compared with the "far-fetched" process of settling of crystals. Possibly the particular reason was the banding of the gabbro. In order to explain the banding it is necessary, however, to assume a simplicity of convection that experience of that phenomenon does not warrant. It is known, for example, that a mass of liquid experiencing convection may divide up into columnar cells of hexagonal section, each of which has its own system of convection characterized principally by vertical currents. While it need not be urged that convection in a large mass of magma would be exactly of this nature, it seems inevitable that a very large number of currents would be set up whose motion would be principally vertical, and that any banding of the rock that might result from convection would be principally vertical. There seems little reason to believe in a great current sweeping over the floor and depositing its load in bands parallel to the floor in the manner Pirsson[1] and Grout have suggested. However, assuming that such a current did exist, it is noteworthy that in order to explain banding it is necessary to make the further assumption of a rhythm in crystallization, such that the crystals brought down by convection vary in their nature as time goes on and may even alternate. And convection is supposed to have a remarkable advantage over crystal settling in producing alternation of layers as a result of this rhythmic crystallization. But while I do not urge rhythmic crystallization I would point out that the crystals brought to the bottom by settling would show precisely the same alternation as those brought there by convection if this rhythmic crystallization can be assumed.

[1] *The Igneous Rocks of Highwood Mountains*, U.S. Geol. Survey, Bulletin 237.

When we come to a consideration of the more intimate details of crystallization we find this same tendency to urge an advantage of convection over settling, an advantage that has no real existence. In the case of the bodies of magnetite, for example, it is pointed out that the best concentrations are centrally placed in the gabbro and not at the bottom, and to explain this it is pointed out that magnetite is, "at least partly, later in time of crystallization" and "would remain liquid until the lower parts of the chamber were filled with rock." Is the explanation not equally applicable to the process of crystal settling? Is there anything in that process that demands the settling of a crystal before it exists? It is true that the writer referred to the accumulations of iron ore as evidence of settling of crystals because they have been described by others as occurring at the base of the Duluth mass, and it is safe to say that in many cases some of the iron ore crystallizes very early, but the writer has always been opposed to the unrestricted acceptance of the notion that all the ore minerals separate early.[1]

We come now to that very special feature of the near-uniformity in the nature of the plagioclase in all parts of the gabbroid portion of the mass, a feature that has been considered to favor especially the idea of convection. Vigorous convection would of course keep plagioclase crystals suspended and in a slowly cooled mass would permit perfect adjustment of their composition to that of the surrounding liquid, so that there would be no necessity of the appearance in the final product of any very basic plagioclase, bytownite, or anorthite. But the rôle assigned to convection by Grout is not so much that of keeping crystals suspended as that of aiding in their accumulation in layers by dragging along the bottom, and, at a time when the rhythm of crystallization was in a phase such that the crystallization of plagioclase dominated, the layer formed is supposed to show a corresponding dominance of plagioclase. These plagioclases, dragging behind and caught in the viscous border, have according to Grout "there slowly, maintaining equilibrium, adjusted their composition to that of the surrounding magma." It should be realized, however, that in so far as basic plagioclases accumulate at a certain place in excess of the amount that could have separated from the liquid at that place, the liquid there surrounding them is incapable of completely making them over into the average plagioclase of the mass.[1] It makes no difference how the excess of plagioclase arrived at that place, the laws of physical chemistry will not be suspended in favor of those arriving by the convection route. The fact that those portions of the mass that are particularly rich in plagioclase do not have significantly more basic plagioclase than other parts may therefore be regarded as a very serious objection to accumulation of plagioclase by convection and indeed to any process involving the accumulation of plagioclase by a positive active motion on its part relative to the liquid from which it separates.

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[1] N. L. Bowen, "The Order of Crystallization in Igneous Rocks," *Jour. Geol.*, XX (1912), 457.

The theory of crystal settling, however, does not demand any positive action of plagioclase, at any rate not until a quite late stage of crystallization. Just as settling is not regarded as possible for a crystal that does not yet exist, so it is not regarded as possible for a crystal unless it is heavier than the liquid. Grout is able to refer to a place (p. 33) where I have discussed the effect of the sinking of plagioclase, but reference to that page will show that there simple mixtures of albite and anorthite are being discussed. Manifestly basic plagioclase crystals will sink in a liquid having the composition of more acid plagioclase, but if femic material is dissolved in the liquid the problem is not so simple, and on another page of the same paper (p. 79)[2] reference is made to this fact. There the following statement is made: "It appears, indeed, that plagioclase crystals may at the earlier stages of crystallization be only very slightly heavier than the liquid at most and possibly even somewhat lighter." In another paper this suggestion is elaborated and the behavior of plagioclase is discussed at some length.[3] It is pointed out that plagioclase "remains practically suspended in the liquid with probably a very slight tendency to rise at first and the whole of the liquid is available for the production of the change of composition that ensues as the temperature falls. Thus, though the earlier crystals of plagioclase are basic bytownite, they are, in nearly all cases, gradually made over into labradorite by the liquid in which they remain suspended." The statements could hardly be more specific, and Grout's reference (with its accompanying objections) to my statement regarding a simple mixture of anorthite and albite is hardly to the point when such definite consideration of the actual case of gabbroid magma has been offered. We have seen then that the specific objections to crystal settling that Grout offers are in part due to a misconception of that process, and that in many cases where he supposes a convective action to have advantages over settling the advantage has no real existence as far as explanation of the phenomena of the Duluth gabbro is concerned. However, it must be conceded that Grout's main contention is substantially correct, viz., that the theory of crystal settling in the simple and generalized form in which it has been stated does not offer an adequate explanation. We have seen that his convective action is likewise inadequate. As an offset to so much destructive criticism a constructive suggestion will be ventured. The suggested explanation will involve two principal assumptions: first, that the igneous mass of the Duluth lopolith was injected as a sensibly homogeneous and completely liquid magma, and secondly, that the action which produced the basin-like form of the mass took place in part contemporaneously with the period of crystallization of the magma. The first of these conclusions is, it should be remarked, in complete accord with Grout's conclusions. Under five heads Grout points out the impossibility of belief in the intrusion of heterogeneous liquid, a process that the writer has suggested as a possible result of the squeezing of liquid from a crystallizing mass.[1] This suggestion should be applied only to cases where the evidence is strong that there were two liquids. I am wholly in accord with Grout that it is a quite inadequate explanation of the banding of the Duluth mass and indeed of most rocks showing primary banding. Usually such banding is to be referred to movement during crystallization.[2]

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[1] N. L. Bowen, "The Problem of the Anorthosites," *Jour. Geol.*, XXV (1917), 212.

[2] These page numbers refer to those in "The Later Stages of the Evolution of the Igneous Rocks," *Jour. Geol.*, XXIII (1915), Supplement.

[3] N. L. Bowen, "The Problem of the Anorthosites," *Jour. Geol.*, XXV (1917), 211.

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[1] N. L. Bowen, "The Later Stages of the Evolution of the Igneous Rocks," *Jour. Geol.*, XXIII (1915), Supplement, pp. 27 and 83.

[2] N. L. Bowen, "The Problem of the Anorthosites," *Jour. Geol.*, XXV (1917), 236 and 237.

We shall begin then with a mass of gabbroid magma wholly liquid and already injected into its present position. When cooling had reached a certain stage crystallization began, and the earliest crystals, olivine and perhaps some titaniferous magnetite, were precipitated. As a result of currents in the liquid, whether from thermal convection or wholly mechanical causes, free settling of crystals was much hampered, so that a large proportion of the crystals remained suspended. In spite of this deterrent action, however, some of the crystals of these early minerals did accumulate on the bottom, where they formed masses of peridotite and presumably also of iron ores in some cases. In the meantime, crystallization of both pyroxene and plagioclase had begun. The plagioclase was at first a basic bytownite. It remained suspended because nearly of the same density as the liquid, and, as cooling and crystallization proceeded, it adjusted its composition to approach labradorite. The free sinking of pyroxene was hampered, presumably by currents, as was that of olivine, so that no accumulation of pyroxenite was accomplished. A certain amount of accumulation of pyroxene did occur, however, together with that of olivine, and the pyroxene crystals augmented the mass of peridotite at the base. There was also a certain part of the liquid from which both olivine and pyroxene crystals had settled out practically completely and that contained only suspended plagioclase crystals.

The mass is now entering a stage when in its lower portions, constituting say one-fifth of the total thickness, crystals are in contact with their neighbors and are beginning to grow together at corners and along edges. This action becomes important when the residual liquid and the crystals have each about one-half the volume of the total mass. In the upper portions the crystals are somewhat more widely spaced, the more so as the height above the base increases, but this feature is not so very marked, because crystal settling has been limited by the lack of quiet conditions. The bottom-most layers including peridotite may be regarded as practically entirely crystalline, with only a small proportion of residual liquid.

Origin of banding.—There is then a tendency toward a layering of the mass on a very large scale but no suggestion whatever of the complicated banding that the finally solidified rocks show. In order to explain the production of banding it is necessary to assume that the action that produced the basin-like shape of the mass was going on continually during the period of crystallization. When the proportion of crystals was small this action could have no effect on the distribution of materials, but when the proportion of crystals began to pass the 50 per cent mark it may have had an important effect.

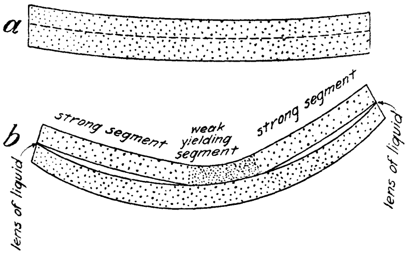

FIG. 4.—Showing the effect of warping on a crystallizing mass: (*a*) shows portion of a mass when it reaches stage at which warping begins to be effective; (*b*) shows the effect of warping causing compaction of crystals locally and the formation of lenticular pools of residual liquid. Density of stippling is intended to indicate the relative, though not absolute, degree of compaction of crystals in various parts immediately after the action. Final result is a banded rock showing that contrast in composition that existed between liquid and crystals at the time of the action.

We shall consider, for simplicity and convenience, the possible results of the bending action as applied to segments of two adjacent layers. The bending will tend to produce a shortening of the upper layer relative to the lower one, and a shortening thrust is therefore developed in the upper layer. This upper layer may reasonably be supposed to have certain portions that are relatively weak and others relatively strong that may be illustrated diagrammatically as in Fig. 4. It may be supposed too that all of the yielding to this shortening thrust will take place in the weak portions of the layers, and that the strong portions will act as practically rigid plungers. The yielding of the weak portions will likewise relieve the strong portions from any bending force, and a strong portion can therefore retain its original shape, with the consequent development of a long, narrow space between it and that part of the lower layer immediately adjacent to it. At first thought it would seem impossible that this space could develop, and if by space is meant empty space this impression would be entirely justified, for the strong segment, though capable of acting as a rigid plunger, would have no real strength as an arch. It should be noted, however, that the strong segment and indeed the whole crystal mesh which we are considering is a thoroughly porous material permitting free passage of the liquid residual magma under its hydrostatic head. A space between the two layers could therefore develop freely, for it would fill with liquid as fast as it opened. The yielding of the weak segment, the opening of a long, narrow space between the bridgelike strong segment and the lower layer, and the filling of this space with the residual liquid would therefore go on simultaneously and indeed would be thoroughly interdependent. The yielding of the weak segment would be the result of the breaking of the relatively weak crystal mesh at the points where crystals had begun to grow together, and once instituted it would continue until the crystals were packed together rather closely with only a little residual liquid, or until the mass had acquired a strength equal to that of the segment that was acting upon it as a plunger. At this time the action would cease as far as the yielding of that particular weak segment was concerned. As a result then of this single episode two layers have been produced whose direction is practically parallel to the base of the igneous mass, one of them having a composition practically equivalent to that of the residual liquid contained in the crystal mesh at the time, and the other a composition not far from that of the crystals that had separated at the time (Fig. 4*b*). If we consider the further history of this particular part of the igneous mass as cooling and further bending proceed it will be found that the segment that had yielded will, when a certain amount of growing together of the already compacted crystals takes place, constitute a particularly strong segment and in virtue of its strength may be the means of production of a layer of the same type as itself, from a relatively weak segment sensibly along its strike, by acting as a plunger and compacting that segment. At the same time the plunger segment will produce immediately beneath itself a band of the opposite type. It is thus seen that the action, once begun, is a means of its own growth and is particularly competent to produce adjacent strongly contrasted bands. Constant repetition of this action as cooling and warping proceed should produce innumerable bands such as those seen in the Duluth mass.

If we return to a consideration of the fundamental action involved, namely the formation of a bridge as a result of the refusal of a certain layer to bend in conformance with the bending of the layer immediately beneath it, we find that this could take place only when the crystal mesh is of such a nature as to have fair compressive strength but very little tensile strength. If the tensile strength were even moderate the upper layer would be pulled into conformable shape by the lower layer, for the tension acts over a relatively large area. The crystals of the mesh must therefore be in contact but must be grown together only to a very limited degree. There is a further reason why the proportion of crystals to liquid must be limited. The bridging action can take place only when the crystal mesh is so open that pressure on the liquid is transmitted to all parts of it with great freedom and with little frictional resistance, for the bridge has no real strength to support a significant superincumbent load, and liquid must be free to flow into the space immediately below it if the potential bridge is to become a reality. On the basis of some rough experiments it is estimated that the action could take place when the mass was 50 per cent crystalline but probably not when the proportion of crystals was significantly less than that value. On the other hand, it probably could not take place when the proportion of crystals was as great as 65 per cent. The contrast between bands is therefore of a different order of magnitude from that shown in the gabbro-granophyre association. There is likewise a rather limited period for any particular portion of a mass during which this action can occur. It is believed that during this period and as a result of the action described the banding of the Duluth gabbro was produced. In this case it is possible to show the relation between the direction of banding and the forces that produced it. For many igneous masses having primary banding this relation would perhaps not be so plain, yet it would seem to be safe to state it as a general proposition that primary banding is produced by the action of warping stresses, torsional stresses, and in some cases shearing stresses, acting at the appropriate stage of crystallization.

Origin of the red rock.—Returning now to the Duluth lopolith, we find it at a stage when crystallization is fairly well advanced.

As a result of continued warping banded structure has developed in successively higher and higher horizons, broadly speaking, though as the top was approached the conditions would vary considerably, and it would seem likely that banding would not be produced as readily. However this may be, we may consider that the stage of possible development of banding has passed, and that the whole mass is approaching complete crystallinity. The lower portions pass through this stage somewhat sooner that the higher portions and finally become completely crystalline, the liquid adjusting itself continually to the composition of the crystals with which it is in contact, until finally all the liquid is used up. In upper portions some interstitial liquid still remains, and principally in those parts from which olivine has been removed by gravity the interstitial liquid becomes of a granitic nature. As the plagioclase crystals, continually adjusting their composition, approach labradorite, Ab_1An_2, the plagioclase content of the liquid approaches acid andesine, and the actual proportion of the interstitial liquid becomes rather small. This is the stage at which a lateral thrust acting on the mass may produce a squeezing upward of residual liquid and a packing together of the crystals after the manner discussed in some detail on an earlier page. It may be noted that in the upper portion of the mass the warping becomes more nearly a simple compressive thrust resulting from the tendency toward the folding in of the two horns of the crescent so that the squeezing out may be regarded simply as a result of a continuance of the warping action and as expressing its result at quite a late stage of crystallization. There is even a very definite suggestion in the shape of the lopolith as mapped of a departure from the simple crescentic form. This departure is, as might be expected, localized where the red rock is developed in force and is of the nature of a bulging of the roof (Fig. 5). Add to this the fact that the plagioclase of the red rock is on the average precisely the proper plagioclase for a liquid in equilibrium with the general plagioclase crystals of the gabbro, and it becomes rather obvious that one is, to say the least, hasty who dismisses crystallization-differentiation as a possible explanation of the gabbro, red-rock association. The red rock has, on the contrary, all the earmarks of a crystallization residuum that could have been squeezed upward by lateral compression of part of the gabbro mass. Grout, in offering objections to crystallization-differentiation as an explanation of the red rock and in discussing in that connection the composition of the liquid, states that "before it reaches the composition of such a granophyre as the red rock of Duluth a large amount of feldspar must have crystallized from a magma too acid to yield basic labradorite."[1] On another page, however, he describes the red rock as amounting to "less than 300 feet of acid andesine." Now the liquid that is in equilibrium with Ab_1An_2 crystals is precisely of the composition of acid andesine, to be more specific Ab_3An_1, so that Grout's objections fall when examined in the light of his own detailed descriptions. If the red rock represented an immiscible liquid it is rather remarkable that at the time at which it had separated it should have a composition appropriate for this final plagioclase composition.

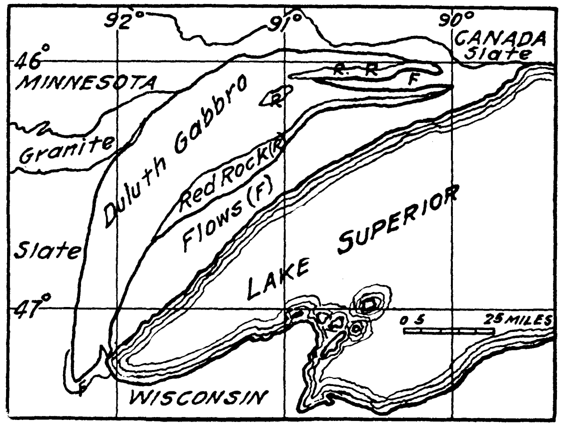

FIG. 5.—Outline map of Duluth gabbro (after Grout)

In connection with this particular association, namely that of granophyre and gabbro, a rather definite suggestion as to conditions of possible immiscibility has been offered. It has been suggested that on account of immiscibility between water and silicates the magma separates into two liquids, the one water-rich and the other silicate-rich.[1] Apart from the objections that have been raised against immiscibility in general, principally with reference to the lack of evidence of the intermediate steps of the process, a further objection should be pointed out that is applicable to this particular type. The contrast between the two liquids should be of this nature, that the one liquid should be mainly a silicate melt with a little water in dilute solution, and that the other liquid should be mainly water with a little silicate in dilute solution. Now, while it is well recognized that the granophyre of the association to which we have reference is characterized by having a greater amount of water than the gabbro, no one would go the length of claiming that the granophyre magma was mainly water with a little silicate in solution. While having, on first thought, some degree of probability the action contemplated does not appear to represent the actual phenomenon.

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[1] "A Type of Igneous Differentiation," *Jour. Geol.*, XXVI (1918), 645.

THE ORIGIN OF NODULES, SCHLIERS, AND RELATED PHENOMENA

Immiscibility has been invoked as an explanation of other phenomena of igneous rocks, principally the formation of monomineralic nodules and of large rock masses of a monomineralic nature. For immiscibility of this nature not even the first-thought probability can be conceded. Substances freely miscible above a certain temperature do not become absolutely immiscible on cooling, with consequent separation of pure end components as liquids. The liquids separating would of necessity be polycomponent solutions, possibly quite strongly contrasted, but nevertheless polycomponent. The liquids must likewise be solutions capable of separate existence at the temperature concerned, and in emphasis of this temperature aspect of the problem it has elsewhere been pointed out that pure molten olivine, for example, can hardly be credited at temperatures near the crystallizing range of olivine basalt. It has been suggested that the liquid need not be pure olivine but could be olivine with volatile components in solution.[2] The obtaining of such a liquid is, however, sensibly the same problem as the obtaining of molten olivine. Any liquid that separated should be polycomponent with respect to its silicate content alone. A volatile component enters the system merely as a component that would distribute itself between the two separating liquids in accordance with a definite partition coefficient which is the ratio of its solubility in the two liquids. Even if we strain a point and assume that one of the liquids could be monomineralic it is necessary to imagine that volatile components distribute themselves between the two liquids in such a way that the monomineralic liquid gets many times as much as the polycomponent liquid in order that the former may remain molten at the temperature concerned. In other words, it is necessary to assume that volatile components are many times more soluble in liquid olivine, say, than in liquid basalt. Apart from the unlikelihood of such a relation the real weakness of the whole case becomes apparent when, in order to explain monomineralic masses of pyroxene, it becomes necessary to assume that volatile components are many times more soluble in liquid pyroxene than in liquid basalt, and in order to explain an anorthositic facies it is necessary to assume correspondingly greater solubility of volatile material in molten plagioclase than in molten basalt. Finally, if it is insisted that this chain of successively dependent phenomena, each fundamentally unlikely, to say the least, may nevertheless represent the facts of the case we may still turn to monomineralic rocks themselves and see if they give evidence of *excessive richness* in volatile components or so-called mineralizers. It was with this reasoning in mind that the anorthosites were examined and found to give no evidence of the presence of mineralizers *in excessive amounts*.[1] Scrutiny of the other monomineralic rocks will, it is believed, yield a like result. The marked serpentinization of peridotites is probably sometimes effected by waters of atmospheric origin (not by weathering), and where accomplished by magmatic solutions there is little reason for believing that these solutions belonged to a peridotite liquid rather than to adjacent igneous masses frequently congeneric with the peridotite.[2]

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[1] Evans, "Congrès Geologique International," *Compte-Rendu* (XXII*e* Session, Canada, 1913), p. 248.

[2] Daly, *Jour. Geol.*, XXVI (1918), 123.

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[1] N. L. Bowen, "The Problem of the Anorthosites," *Jour. Geol.*, XXV (1917), 231.

[2] Benson, *Amer. Jour. Sci.*, XLVI (1918), 710.

It has been considered that the nodules consisting mainly of a single mineral and occurring fairly frequently in basaltic rocks may represent immiscible liquid globules. Olivine nodules in basalt are perhaps the commonest examples. The term nodules is rather misleading, for they are characteristically angular and justify the conclusion of a great number of petrographers that they are fragments torn from a mass of olivine rock (Olivinfelseinschlüsse). Rosenbusch, however, always insisted that they represent intratelluric crystallization from the rock mass in which they are found. As a matter of fact it is more reasonable to suppose that both conceptions are true, and that the nodules represent fragments torn from an olivine rock that was formed from the basalt by crystallization-differentiation. It is not necessary to suppose that the olivine settled to the bottom of an intratelluric mass of basaltic magma; indeed the characteristic occurrence of the nodules in narrow dikes and small necks of basalt suggests an action that may be of considerable importance in petrogenesis. The suggested action is the formation of crystal jams, or the clogging of the channels occupied by moving magma as a result of the packing together of crystals at a constricted point, in much the same manner as log jams and ice jams are formed in rivers. Any marked constriction of a channel in which a liquid is flowing that contains suspended solids, even in very small amount, is likely to cause such clogging. The formation of a crystal jam is highly probable in a narrow dike or neck through which basaltic magma is flowing with upward of 10 per cent olivine crystals suspended. Once formed the clogging may result in the accumulation of a considerable mass of crystals, and the passing of liquid through the mass, a liquid from which olivine and only olivine is being precipitated as yet, may cause the growth of the crystals of olivine until no interstices are left. Accumulation of pressure on the magma, aided perhaps by terrestrial disturbance, may then break the barrier and carry the olivine rock along as inclusions of the nature of olivine nodules, some of which may become rounded as a result of the resorption that olivine normally suffers. While the clogging of a channel with very early crystals, such as olivine, is to be expected only in small channels, there is reason to believe that even large channels might be similarly affected at later stages of crystallization, and in this case, while the disrupted mass would not give monomineralic inclusions, it might readily give inclusions strongly contrasted with the magma that would produce schliers and related irregularities in the rocks formed.

In the banded peridotites of Skye, Harker describes seams of nearly pure picotite and chromite. It is possible that these may have been formed by some such action as that described above. However this may be, the petrologist should look askance at molten spinel and chromite. Molten corundum, molten olivine, and molten magnetite even with volatile components in solution are to be similarly regarded, and every possibility of explaining masses of such minerals as the result of crystal accumulation from complex magmas or of hydrothermal action should be exhausted before the probability of such melts is entertained, for the chemical difficulties in the way of their production are colossal. By hydrothermal action is meant introduction by dilute solutions as contrasted with introduction as a melt, even though it be a melt with volatile material dissolved.

ORIGIN OF THE ALKALINE ROCKS

Throughout the foregoing pages consideration has been given to phenomena exhibited only by the so-called subalkaline rocks. For such rocks we can speak with considerable assurance regarding the chemical effects of relative movement of crystals and liquid, because systems approaching them reasonably closely in composition have been completely investigated. Concerning the alkaline rocks we cannot speak with the same assurance, for not much more than a beginning has been made in their experimental investigation. Nevertheless some consideration will be given to the alkaline rocks, particularly to the question of the relationship between alkaline and subalkaline magmas. The writer has stated elsewhere the opinion that the most important of the alkaline magmas, foyaitic magma, is definitely related to granitic magma. The equilibrium reactions which take place in granitic magma, and of which the evidence is plain from the occurrence side by side of feldspars, mica, and quartz, furnish a clue to the origin of foyaitic magma. The principal reactions indicated are the breakdown of polysilicate molecules into orthosilicate molecules and free silica, and they may be written empirically as follows:

$$KAlSi_3O_8 \leftrightarrows KAlSiO_4 + 2SiO_2,$$
$$NaAlSi_3O_8 \leftrightarrows NaAlSiO_4 + 2SiO_2.$$

Certain other reactions no doubt occur, such as

$$H_2O + NaAlSiO_4 \leftrightarrows HAlSiO_4 + NaOH,$$
$$H_2O + KAlSiO_4 \leftrightarrows HAlSiO_4 + KOH,$$

which are simple examples of hydrolysis. That such reactions are going forward in the liquid is attested by the fact that certain of the products are precipitated, the polysilicate molecules as feldspar, the orthosilicate molecules $KAlSiO_4$ and $HAlSiO_4$ in mica, and free silica as quartz. Evidently certain other products not precipitated are stored up in the liquid, and the principal of these is $NaAlSiO_4$, the liquid becoming highly alkaline and showing a high concentration of water and other volatile components when finally precipitation has taken place to such an extent that the liquid is a very small proportion of the total mass. The compound $NaAlSiO_4$, the distinctive molecule of foyaitic magma, can be precipitated from this liquid only if it is removed from the presence of quartz. In the presence of quartz the reaction is simply reversed, and the polysilicate feldspar is precipitated instead of the orthosilicate nephelite. It is not impossible that rapakivi structure and certain perthites whose albite lamellae appear to have been formed by reaction may represent the stage of reversal of this reaction. It is not essential that such structures be present, however, for commonly the using up of this liquid would be accomplished merely by continued growth of albite or closely related plagioclase crystals. There does appear, however, to be a particular tendency toward a precipitation of albite later than that of orthoclase (rapakivi-like though not necessarily having the particular rapakivi structure) in granites that are related to nephelite syenites. It should be noted, however, that this highly alkaline and highly aqueous liquid that is thus formed as the crystallization of granite becomes far advanced is a very fugitive material, and that the crystallization of a large mass of granite must proceed very slowly at this stage.

Throughout a long period this liquid is therefore more or less free to escape into the surrounding rocks, where it is used up in producing metasomatic reactions of various kinds. The materials of this liquid may therefore be frequently, perhaps normally, unrepresented in the igneous sequence for this very reason. Foye has brought forward quantitative evidence of the introduction of alkaline material in great quantity from granitic magma. Following Daly he imagines that these alkaline liquids have been produced by reaction between limestone and granite, but it is not at all clear how this can be considered to apply to the particular case under discussion. In its original form the limestone-syntectic hypothesis was rather simple, and, as the writer has stated elsewhere, it assumed rather likely reactions. The most important reaction was the taking of silica by the lime to form lime silicates, the free silica being used up first and later some of the silica of feldspar molecules with consequent production of feldspathoids. However, it is imagined in this particular case that limestone reacts with granitic magma but does not desilicate it, in fact leaves it still a granitic magma, and yet reacts in such a way as to produce large amounts of alkaline liquors, of which part are introduced into the invaded limestones and part crystallize with formation of nephelite syenite. The manner in which this can occur is by no means so easily followed. On the other hand, it appears to afford a natural explanation if it is supposed that biotite granite magma is normally capable of supplying this alkaline liquid, and that it is produced by reactions of which we have direct evidence in the granite itself. The liquid may be used up by introduction into surrounding rocks or by partial reversal of these reactions if it remains in the interstices of the granite. On the other hand, if it is separated from the granite in such a way as to form a distinct liquid it will give rise to a mass of nephelite syenite and may indeed further differentiate to give derivatives of nephelite syenite.

In the paper in which this mode of origin of nephelite syenite was proposed it was stated that a continuance of the process of settling of crystals could produce nephelite syenite from granite in the same manner as granite itself may be produced from gabbro. This manner of separation of the liquid from the crystals is not now considered likely, indeed the essential reactions have probably not proceeded adequately until a late stage in the crystallization of granite, when crystal settling was no longer possible. The only method of separating the liquid at this stage is the method of the squeezing out of residual liquid by the breaking down of the crystal mesh under the action of outside forces. If this be true, alkaline magma can be produced only as a result of the action of outside forces, whereas subalkaline magmas, while they too can be so generated, may nevertheless be generated spontaneously by crystal settling. It is possible then that the alkaline rocks might show a tendency toward a definite relation to earth movements[1] not exhibited in the same degree by subalkaline rocks, but the writer is unable to see at present what the details of this connection may be.

The succession of igneous rocks deduced from the foregoing considerations—from basic through silicic to alkaline—has been discussed in some detail elsewhere.[2] An objection to this has been raised on the basis of the fact that in the Pacific basin there are basic rocks and alkaline rocks and none of the intermediate silicic rocks.[3] It should be noted, however, that these objections are based entirely on the evidence of effusive rocks and the occurrence of a magma, as an effusive mass depends on certain factors other than its generation within the crust of the earth. Daly has, for example, presented figures that show that granitic material is twenty times more abundant than basaltic as deep-seated masses, whereas basaltic material is fifty times more abundant than granitic as effusive masses. There is therefore something about granitic magma that limits very seriously its occurrence in effusive types, even though it may be abundantly present as subjacent masses. We need not go into the question of what these factors may be, though some readily suggest themselves. The significance of the fact in this connection is that the types that happen to be exposed in an effusive sequence may give but a poor indication of the genesis of their magmas, and it may be taken as a general rule that fundamental relationships are much more likely to be deducible from plutonic masses. When we turn to the evidence of plutonic masses on this question we find that they speak in no uncertain voice regarding the intimate connection of silicic with alkalic rocks.

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[1] Such as that suggested by Harker in the classification into Atlantic and Pacific types.

[2] "The Later Stages of the Evolution of the Igneous Rocks," *Jour. Geol.*, XXIII (1915), Supplement, pp. 55 ff.

[3] Holmes, *Science Progress*, XI (1916–17), 68.

This association of silicic and alkalic rocks, we have seen reason to believe, is of fundamental significance and is a fact very difficult of explanation on the basis of the limestone-syntectic, desilication hypothesis. Ordinary granites, nordmarkites, and highly siliceous alkaline granites are world-wide associates of nephelite syenites.

In yet another respect the limestone-syntectic hypothesis lacks support. The proponents of syntexis as a general phenomenon frequently support their claims by pointing to the fact that fragments of siliceous rocks immersed in gabbro frequently show rims of a granitic nature about them, and that fragments of quartzite immersed in nephelite syenite may show rims of alkaline granite. These and like observations at least demonstrate the possibility of certain reactions, though one may still doubt their quantitative importance in petrogenesis. In the case of the reaction between feldspar-bearing magma and limestone to produce feldspathoid-bearing magma no support of this kind is to be found. Magmas of all subalkaline types have at times included blocks of limestone that have been made over into lime-silicate rocks, yet in no case are these inclusions surrounded by rims of alkaline rock. The limestone-syntectic hypothesis lacks, therefore, even that measure of support that is afforded syntexis in other cases through the occurrence of the reaction aureoles mentioned.

SUMMARY

In this paper some of the objections that have been raised against the theory of crystallization-differentiation are considered, and its adequacy to explain certain phenomena, for which it has been considered to fail, is pointed out. The supposed advantages of liquid immiscibility in explaining discontinuous variations are considered, and reasons are given for believing that no such advantages exist. On the basis of crystallization explanations are suggested for discontinuous variations, particularly those noted in the association gabbro-granophyre. A suggestion is made as to the origin of primary banding with particular reference to the Duluth lopolith.